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COMMENTS

COMMENT ON "GAPS AND GLISSANDOS . . ."

(Comment on Tyree et al., *ASR*, June 1979)

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Tyree, Semyonov and Hodge (1979), hereafter TSH, examined the relationships between social mobility, economic development and the "shape of the social stratification system." They concluded that social mobility has a close association with the latter, of which its association with economic development is a spurious consequence. This comment points out that their social mobility data was incorrectly transcribed from the source articles for four of the 24 countries with major errors in each case, and that the regression analysis on which their assertion is based is unsound. A reanalysis leads to different conclusions. The relationships between mobility and TSH's explanatory variables differ markedly between the developed and the underdeveloped countries. Mobility seems to vary with GNP in the same way as income inequality: first decreasing then increasing with GNP.

DATA

TSH used a mobility measure which they said was based only on urban sons of urban fathers. Their sample size $N = 8950$ for the Canadian data from McRoberts et al. (1976: Table 1) wrongly includes class VI (farmers); the correct value is $N = 5201$. For the Philippines data from Bacol (1971: Table 1), they give $N = 8892$, but excluding fishermen and loggers gives only $N = 5492$ on the basis of Bacol's "weighted sample size," and reduces the log odds ratio from 2.25 to 1.94. For Puerto Rico, using Miller (1960) they report $N = 1785$ which includes agricultural day labourers; excluding them leaves $N = 795$. For Hungary, based on Andorka (1971: Table 1) they take "office attendants" to be white collar, which contradicts Table 7 of the source as well as being implausible, and reduces the log odds ratio from 1.87 to 1.57.

Table 1 gives revised log odds ratios for the 24 countries. TSH gave a linear function of the log odds ratio, but here we give the log odds

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ratio itself as it is easier to reproduce. We also give the per cent foreign born (pcfb) for each country which TSH used but did not tabulate, expanding their coverage using the 1977 UN Demographic Yearbook.

REGRESSION ANALYSIS

TSH's conclusion is based on two regressions, one of which relates mobility to GNP and a measure of income inequality, while the other relates mobility to GNP and the proportional size of the middle class (midocc). In standard form the revised equations are

$$(1) \text{ mobility} = - .279 \text{ GNP} + .579 \text{ inequality}$$

$$(.175) \quad (.175)$$

$$R^2 = .59$$

$$(2) \text{ mobility} = - .045 \text{ GNP} + .680 \text{ midocc}$$

$$(.261) \quad (.261)$$

$$R^2 = .51$$

with standard errors shown below the coefficients. As in TSH, "mobility" is *inversely* related to the chance of moving. Replacing GNP by log GNP makes little difference. (1) and (2) resemble TSH's equations but the values of R^2 are lower.

Table 1. Social Mobility Log Odds Ratios and Per Cent of the Population Foreign Born (p.c.f.b.): 24 Countries

Country	Mobility	p.c.f.b.
Australia	1.28	18.4
Belgium	1.97	7.6
Brazil	2.37	2.3
Canada	1.06	15.6
Chile	2.03	1.4
Colombia	3.25	0.3
Denmark	1.69	2.1
France	1.62	8.2
Great Britain	1.43	5.2
Hungary	1.87	6.8
Israel	0.70	44.9
Italy	2.16	—
Japan	2.03	0.6
Mexico	2.04	0.6
Netherlands	1.68	3.9
Norway	1.87	1.7
Philippines	1.94	0.2
Poland	2.10	6.4
Puerto Rico	1.59	2.7
Spain	2.08	1.1
Sweden	1.66	4.0
United States	1.39	5.4
West Germany	2.11	—
Yugoslavia	1.76	0.8

Equations (1) and (2) are badly flawed in two ways. First, the residuals for two countries, Israel and Colombia, are very large and may be outliers. Leaving them out changes (1) to

$$\begin{aligned} \text{mobility} = & \\ & -.532 \text{ GNP} + .346 \text{ inequality} \\ & (.180) \quad (.180) \\ R^2 = & .59 \end{aligned}$$

Now GNP is highly significant while inequality is hardly significant. This is the opposite of what TSH claimed to have established and shows that inferences based on (1) are unstable.

The second problem is that leaving out important variables, e.g., in (1) *midocc* and *pcfb*, can seriously bias the estimates of the coefficients and the residual variance, as pointed out by Johnston (1972:169). Indeed the residuals from (1) are significantly correlated with *pcfb* ($r = -.57$), and so inferences from (1) are invalid. The same is true of (2).

To assess the effect of the four variables we must start by including them all in the regression, which increases R^2 to .78. The coefficient of GNP is still small and insignificant, and the model is more stable than (1) in that leaving out Israel and Colombia does not change this. The only variables to make large contributions are inequality and *pcfb*. It may be objected that inequality and *midocc* were intended by TSH as alternative measures of the same thing and that including them both would introduce multicollinearity and reduce the significance of the underlying structural variable. However, the theoretical justification for this seems tenuous and, in fact, as we see below the correlation between them is small.

RESULTS BY LEVEL OF DEVELOPMENT

However, such regressions do not allow valid inferences to be drawn because the dependence of mobility on the four variables is different among the developed and the underdeveloped countries. Drawing a line through the GNP figures designates Brazil, Chile, Colombia, Mexico, the Philippines, Spain and Yugoslavia as underdeveloped (in 1965). The other 13 countries for which we have complete data are considered developed, and for these

$$\begin{aligned} (3) \text{ mobility} = & \\ & -.212 \text{ GNP} - .102 \text{ ineq} - .463 \text{ midocc} - .579 \text{ pcfb} \\ & (.118) \quad (.092) \quad (.157) \quad (.134) \\ R^2 = & .95 \end{aligned}$$

The fit is very good and there are no large residuals. The only variables to make significant contributions are *midocc* and *pcfb*, but there is no basis for asserting that *midocc* is the main correlate. Indeed, even the reality of the

association is open to doubt as it may be a spurious consequence of other, e.g., demographic and educational variables, which are important for mobility.

For the underdeveloped countries things are less clear but markedly different. Here

$$\begin{aligned} (4) \text{ mobility} = & \\ & 1.010 \text{ GNP} + .962 \text{ ineq} - 1.035 \text{ midocc} - .110 \text{ pcfb} \\ & (.672) \quad (.372) \quad (.617) \quad (.323) \\ R^2 = & .82 \end{aligned}$$

Only *pcfb* can be left out without reducing R^2 considerably, and so (4) does not allow us to assert that GNP is unimportant for mobility. The coefficient for GNP is positive, while in (3) it is negative, suggesting that at a low level of development, greater productive capacity may be associated with *less* mobility rather than more. This would result if mobility first decreased and then increased or remained stable as GNP increased. A similar phenomenon for income inequality was first noticed by Kuznets (1955) and more fully documented by, e.g., Fourastié and Bazil (1980:214). Of course the coefficient of GNP, while large, is not quite statistically significant. This may well be because (4) is based on only seven observations, and further data would be needed to reach firm conclusions.

In (3) the correlation between inequality and *midocc* is a nonsignificant $-.40$ and in (4) it is only $-.26$. Thus the inclusion of both does not cause multicollinearity. This also casts doubt on TSH's view that they are alternative measures of the same thing, i.e., of how continuous a social structure is.

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SOCIAL MOBILITY AND IMMIGRANTS
OR IMMIGRANTS AND
SOCIAL MOBILITY*

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Raftery raises three issues in his comment. First, we got some of our four-fold tables wrong—including farm fathers when we said we didn't. He seems to be right—and for two populations, the Philippines and Puerto Rico, our errors are consequential. Our Canadian error turns out not to be so. The difference between his and our collapsing of the Hungarian data can well be seen as a matter of honest disagreement—one on which we are willing to yield if he sees the matter as important. For reasons he does not indicate, he also gets a different value for mobility in Yugoslavia. In general our measures and his are in agreement and, as he reports in his equations 1 and 2, one gets rather the same results with one set as with the other.

The second issue Raftery raises has to do with an additional variable: we did not talk about immigration. He is not entirely correct here. We devoted two columns of text to two asides noting that the four countries with the most circulatory mobility have histories of providing permanent homes to unusually large numbers of immigrants relative to their sizes. We presented zero-order correlations between structural variables and percent foreign born and speculated about their interpretation.

We did not pursue the matter in the article because we could not plausibly include immigration or percent foreign born in a causal logic promoting differences in circulatory mobility. We saw immigrants not as a given that influences how a social order functions, but as people attracted differentially to countries. In this context the dependent variable of our original article, circulatory mobility, is inde-

pendent, a characteristic of a social order that may attract or repel prospective international migrants.

For most of the countries in the small sample Raftery and we share, the percent foreign born lies between 1 and 7 percent. The only prevailing theory by which such a small body of people could be a major determinant of social mobility argues that persons at any one point in time will be ranked socially by their arrival temporally, with first comers on top, late comers below. This mobility is structural, not circulatory. This kind of immigration, understood by this theory, cannot explain any variance at all in circulatory mobility.

That immigration can influence rates of circulatory mobility, we, with Raftery, suspect is true. We do not think in general the effect is powerful. We do not regard Raftery's equations as evidence on the matter. Immigration is only another way to "reproduce" a population. Its effects on mobility must depend on the status of the positions the immigrants occupy—and this varies substantially across countries.

That people thinking of emigrating regard some destinations as more attractive than others is incontrovertible: they say so to journalists, to friends, to us. Their freedom of choice is hardly absolute.

The less than voluntary aspect of international migration has received extensive notice. The mid 19th century emigration from Europe was essentially a Malthusian evacuation of the area. The decline of colonialism meant the repatriation of colonists, especially French, Belgian, Spanish, and English. The attainment of independence by previously colonial countries and changes in their governments since have prompted extensive displacement of peoples, often across the nearest friendly border. Wars predictably produce refugees.

Just as emigration is not always voluntary, immigration has its constraints. The various U.S. Quota and Immigration Acts restricting access to the United States have their counterparts in the immigration laws of other countries.

One need not subscribe to a view of migratory freedom to suspect some countries are more attractive than others to migrants and this is reflected in the size of immigrant populations. Surely one facet of attractiveness is an immigration policy liberal enough to permit entrance. Another is likely to be an emigration policy permitting departure if the move turns out an unhappy one.

What else might make one potential destination more attractive than another to an international migrant is a matter puzzling students of international migration. In the context of

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